Review of:
Consideration of Long-Term Climatic Variation in SFWMD Planning and Operations by Obeysekera et al, June 4 2006,

Rafael L. Bras, reviewer
July 2, 2006

Answer to questions

This review first answers six predetermined questions provided by the South Florida Water Management District. In answering, the reviewer does not imply endorsement of the choice of questions. General comments follow the discussion of the pre-specified topics.

Question 1: Has the District adequately addressed the long-term wet and dry cycles in modeling for a) facility planning, and b) operational planning. If not, what standard engineering practices can the District modelers follow to address climate variability due to indicators such as AMO.

The district is utilizing standard engineering practice. The planning and operation seems to be based on a well-conceived and quality-controlled data set of rainfall (1965-2000) that does exhibit variability and fluctuations that are representative of what could be expected. The system modeling tools are appropriate and well sanctioned. Having said that, the report does not resolve, the issues related to unusual inflows into Lake Okeechobee, particularly during the last few years. The analysis is still ambiguous. This will be addressed later. Given this observation, it is apparent that an additional effort to understand the climate fluctuations is necessary. It is also appropriate to perform additional modeling using synthetic sequences of precipitation and/or inflows.

Question 2: Is there compelling evidence that the volume of inflows to Lake Okeechobee will be as much as double during a wetter cycle as they were in a dry cycle? In the current modeling efforts, has the District adequately addressed the variability of inflows into Lake Okeechobee?

There is compelling evidence that very high lake levels have occurred recently and in the past. That this is due to particular climatic oscillations is less evident. I have several comments about the way the report handles the issues related to climatic cycles.

1. The report focuses on AMO although acknowledges that other shorter and longer term oscillations may play a role in enhancing or diminishing impact annually or variably in seasons. What is the net effect if any? I understand that the focus on AMO responds to public comments but the discussion should not be limited to that particular climatic index.

2. There is a confusing use of the term “climate change’. This term, for better or worst, triggers thoughts of anthropogenic climate change, which is not what is being addressed.
3. It is clear that the issue here is one of seasonal variations, largely the months around the September-November period, and furthermore it is more an issue of inflows than of rainfall. The report could be far more focused on that issue. At the end of the day, it is not possible to formulate a definitive opinion based on the information given. The mechanisms that lead to the very high inflows are not elucidated. It is not clear if indeed the seasonal rainfall increases can cause the observed net inflows or if, as implied, they can be due to changes in operation upstream of Lake Okeechobee. A comparison of the distribution of net inflows by month or season (not just annually like Figure 5) is needed.
4. Most comparisons, certainly the statistical ones, were made between the AMO2 (1926-1969) and AMO 3 (1970-1994). Tables 5 and 6 of Appendix A are exceptions and the show that little difference in monthly rainfall between the (approximately) AMO2 and the design period (1965-2000) It does show some statistical significance in inflows. Why? Why not use the exact definition of AMO periods for the comparisons? I noticed that AMO 4, albeit short, is never compared, except in Table 3 of Appendix A and that, on the surface, seem to indicate as much percent change between the two warm/wet periods, AMO2 and AMO4, as between wet-dry, AMO2 and AMO3. Again, annual comparisons may be uninformative.

The report is ambivalent about its findings. On the one hand it seems to be arguing that there is too much uncertainty in these climate oscillations to use them in planning and design (a potentially defensible argument). On the other hand it seems to argue for tools to use the forecasting of extremes using the cycles. The position is akin to a reasonable precautionary principle but, as presented, it is confusing and promotes more doubts that precaution.

Question 3: Does the modeling approach used by the district for both CERP and WSE schedule design meet requirements of standard engineering and design practices. If not, what additional steps should the district take to improve modeling for these purposes?

Without prior knowledge of the 2x2 SFWMD this question cannot be answered. The reviewer does have that knowledge so it is possible to say that the tools used by the District and the approaches used are appropriate and meet requirements and standard engineering practice. The motivation of this report is interpreted as investigating the appropriateness of the input data used in the modeling, not an assessment of the modeling approaches that it cannot possibly do justice to. Is the input data appropriate? Figure 10 of the main report is convincing that the historical data used is fairly representative of natural variation in precipitation. I also agree, although the report is not as clear as it should be, that designing based on some sort of prediction using every climatological cycle is probably not advisable. But it is possible to present better information on sensitivity to climate variability and to discuss its

impact on design and operation. It would be desirable to have a more detailed study of seasonal inflows and lake stages. Figure 11 gets close to this and indicates that on an annual level a 10% precipitation change can lead to significant lake level increases for the more rare years. But, what about seasonal, non-uniform, changes in precipitation? What will the impact on lake stage be? That is the question that needs to be better explored.

Question 4; Are the steps being taken in the adaptive management/modeling approach used by the District adequate to address the uncertainties in climate predictions and to assimilate new information?

As stated above, the modeling approach can be supported based on past knowledge. Position analysis and the WSE schedule determination are also endorsed for use, based on prior knowledge. It is unclear what "adaptive management" is, based on the one paragraph on page 28. In a way not much more is needed to state that the District is being cautious and constantly monitor, evaluate and adjust their decisions. That is wise, climate oscillations or not.

I do think that the report should present a more thorough sensitivity study of the impacts of using different historical or synthetic records on the designs and operation of the system.

Question 5: Except for basic research approaches, are there other facility planning options that the District should consider to address the possibility of continued wetter cycle?

The District has all the appropriate basic tools. Some of the report recommendations are appropriate responses to this question, particularly:

1. Continue to incorporate new climatological data
2. Investigate alternative methods to extend the modeling period backwards, prior to 1965
3. Refine the rainfall-Lake Okeechobee inflow relationships.
4. Continued monitoring and evaluation of climate and their impact on design, construction and operation of projects.
5. Use synthetically generated rainfall data to account for climate indicators.

In carrying out recommendation 2, it may be worthwhile to accept that going back in time will necessarily mean approximations but that the uncertainties of those approximations (i.e., missing spatial resolution) may be quantifiable and that the uncertainty could be propagated to the output.

Recommendation 3 must deal with understanding the seasonal behavior of inflows. What causes the unusual inflows and stages in the lake? You must be definitive about the impact of the management of basins upstream of the lake. The equivalent, but seasonal, version of Figure 11 of the main report must be obtained. The argument that the high points in figures 6,7,8 of the main report are outliers is not defensible. That is

the main point of the study, to explain that behavior. More revealing, and not explored, is why there is little difference between AMO3 and AMO4 in the same diagrams.

Recommendation 5 is a good one but requires more analysis than presented. An unavoidable conclusion of the report is that differences in time periods are, whether due to climate oscillations or not, seasonal. They occur sometime near the end of the wet season, probably between August and November. Statistically, the differences seem to be marginal in precipitation and more significant in inflows, during the months mentioned above, see Table 5 and 6 of Appendix A. Based on the information available it is valid to wonder whether indeed this is an effect associated with tropical storms and hurricanes, towards the end of the tropical storm season. Storage effects of various types and natural delays could account for the observed high inflows lagging a month or two the bulk of the season. This must be ascertained by separating the rainfall of tropical storms from that due to other mechanisms. The climatological analysis must be done for the potentially different populations. If there are different behaviors observed, then any synthetic generation of rainfall must take the two population differences into account. The District must also show that their rainfall-runoff modeling captures the response to large rainfalls, including antecedent conditions, properly.

Question 6: Are data and models used by the District appropriate (reasonable and adequate) for their intended applications?

This has already been addressed in answers to previous questions. In a nutshell, using previous knowledge it is possible to state that models are appropriate and adequate. The use of historical data is always the wise thing to do if the record is long enough and captures the variability well enough. The report is convincing that the 1965-2000 period spans the variability of precipitation well, even seasonally. It is less convincing in explaining the variability in seasonal inflows and the ability of the District's models to capture those seasonal inflows. Clearer evidence is needed that the model can reproduce the high stages in Lake Okeechobee corresponding to the periods of higher seasonal rainfall and that these were properly simulated for design and planning purposes. Given the seasonal storage in the system it is advisable that sequences of probable seasonal rainfall, synthetic sequences, be used to test the robustness of the designs (see answer to question 5 above).

General comments: issues of concern that should be addressed

Although the report has all the elements to address the subject implied in the title, it is weak in its clarity. Following are a series of impressions, some which may be repeated or similar to comments made in response to questions.

1. Despite the general title the report deals only with the Atlantic Multidecadal Oscillations despite acknowledging that it may not have the strongest signal in

comparison with other climate indicators (ENSO, PDO), and despite acknowledging that if these effects are real they may enhance or inhibit each other depending on their relative phases and the nature of the impact. The recommendation is to deal with the broader set of issues or clearly narrow the scope of the effort. It may be that for operations, ENSO is more important.

2. The report spends too much on annual effects when clearly the impact, if any, is seasonal.
3. The report "suggests that water management activities such as Kissimmee basin regulation, not just climate effects, likely have effects on the seasonal distribution of volume of water discharged to the lake." This is a key statement and the report must do more than simply suggest. It either is or is not, that is the key to the AMO issue.
4. The use of the words "climate change" is confusing given that it commonly triggers visions of anthropogenic origins to change, which is not the subject of study. The authors do mix the anthropogenic issues from time to time, which is not appropriate in this report since it is not its focus of study. The definitions of climate shifts, changes, weather anomalies, etc. seem more complicated than needed.
5. The report commonly uses terms like "recent studies" but provides no attribution of references in the text. This has to be done.
6. There are locations where the report takes too much liberty like stating: "Wet regions of earth could become much drier and dry areas could become much wetter." This may or may not be true but has nothing to do with the goals of the report and there is no evidence or reference backing up the statement.
7. Generally a "phase change", at least in mathematics and signal analysis refers to a shift in a periodic signal, not to a being in a different state of the signal, which is what is meant here.
8. The rigorous definition of the AMO needs to be provided (Fig 1 of main report). What observations are used to define the index? How is the reference temperature fixed?
9. Mention of models (ANN) employed to predict net inflow into Lake Okeechobee using climatic indices is not backed up by how useful these models are. It seems that doing so is crucial to the topic of this work.
10. In page 7 it says that the average length of an AMO cycle is 60 years. That means that between years 1870 to 2005 you would have seen, barely, 2 complete cycles. Not enough for statistical inference. Then the report says that tree ring data (what is the error expected in finding ocean temperature anomalies based on tree rings?) leads to an average length of the warm period to be 34 years (ranging from 11 to 60 years). Does the 60 years truly refer to a wet and dry complete cycle or is there and inconsistency here?
11. What does a "simulated reconstruction" of annual rainfall patterns (Figure 3 of main report) mean?
12. Page 9 states "the AMO seems to have its greatest effect during wet season (May through October) months." A few sentences later and in the Appendix it says" statistically significant shifts in monthly distributions of rainfall occur during September through November…" Which one is it?

13. The conclusion “In view of the uncertain …it is risky to assume that the current AMO warm phase will last for any pre-defined number of years or that it will result in consistently higher rainfall conditions within a cycle” is probably true but not well supported at the time the statement is made in page 9.
14. Is it obvious that the evapotranspiration is much lower during the dry season? Not a general statement in my opinion.
15. There are two definitions of net inflows in page 12. As written it is a little confusing. It is apparent that net inflows were computed based on subtracting outflows from change in storage. What would have happened if they were estimated as precip+inflow-ET (first definition)? Can you prove consistency in the data?
16. The key here is to study the seasonal inflows. A diagram like Fig 5, but seasonal, is needed. How does the design period compare then? This is partly answered in the tables of Appendix A. Seasonal behavior is the key to the argument. Follow up on the statement “ warm phase rainfall is concentrated in more intense events toward the end of the wet season [author note: this is not a terribly strong signal], then the overall effect on net runoff near the end of the wet season may be dramatic.”
17. In page 13, define the percent reductions better. You refer to “other factors” then begin the list with AMO! That is not “other”. The other 2 factors, particularly the regulation of the Upper Kissimmee Basin is serious and important but nowhere it is this followed up, proved or disproved. This is very important and without a definitive statement nothing can be concluded.
18. Figures 6,7 and 8 are a step in the right direction of dealing with the issues but the data is analyzed at the annual level, rather than seasonal and the report dismisses the most important AMO2 extremes as outliers. This is what needs to be explained! Those points are the key. [Note that the captions of Figures 7 and 8 seem reversed or the axes are mislabeled. Figure 7 is not “ percentage of runoff due to rainfall”.].
19. “However, implications of changes in tributary basin management need to investigated”. Yes! Until that is done the statement at the bottom of page 15 is not warranted.
20. The first paragraph under “Planning” in page 16 may be true but has no basis on the text. In fact, if it is true then why is the report is exclusively dedicated to a single indicator and does not account for any delays in responses of the atmosphere? The last sentence of that paragraph [‘seemingly minor changes in … rainfall distribution may produce dramatic changes in hydrologic variables such as runoff.”] is the key. Are you sure? Have you proven it? This is what needs to be quantified.
21. The whole discussion of planning and management models makes sense only if the reader is aware of the models and can judge based on that knowledge. There is not enough information here for the neophyte to judge.
22. Statements like expanding the range of years used in simulations “are limited by the fact that it is difficult and probably not beneficial” are not supported. Difficulty is not a reason and benefit has not been proven or not proven.

23. Figure 10 is a good and convincing argument of the representativeness of the data, although it is dependent on a data set PRISM that is just another estimate. A sense of how good is this estimate for the region in question would be useful. It would be good to see the same type of comparison for seasonal inflows into lake Okeechobee.
24. Figure 11 needs to be repeated for seasonal elevations (September-November). The effect on lake stage of 10% annual precipitation changes is large. This type of sensitivity study needs to be expanded.
25. The statement, in page 22, "by using standard engineering practices to design a project for the 100-yr event or the SPS, most extreme events are incorporated into the design, whether or not those events were included within the range of modeling simulations" is either wrong or misleading. The 100-year flood estimate has to come from a distribution assumption that is presumably influenced by the range of observations.
26. If risk is used for water supply, page 22, why not use it for flood control (see above)?
27. Page 23 says that USACE has approved the use of AMO and ENSO to predict seasonal and multi-seasonal outlooks for operational planning. If things are so uncertain as argued in other parts of the report, why do this? Isn't this using questionable and weak climatic links?
28. The report says that only ENSO is important and justifiable given the short record, Then, why the statement about AMO above?
29. Again, why the statement in page 24 about the use of indices in WSE if the uncertainties are so high?
30. Bottom of page 25, what are the "drier" hydrologic conditions?
31. The sea level rise issue mentioned on the bottom of page 26 has no reason to be here. Sea level rise is not the issue in this report. This whole section seems out of place.
32. There are no climatic indicators in the section of facilities planning and design considerations (page 27) although that is the title of Section IV where it resides. The whole discussion of cost and value is unwarranted and out of place. This is simplistic and without justification.
33. If ENSO (page 29) has the strongest effect in Southern Florida, why it was not discussed?
34. Recommendation 4 is confusing. Isn't this whole report intended to show that the record used was representative of the range of rainfall conditions, then why recommend to go beyond it now? This is a two-sided position.
35. What is the point of recommendation 5? The argument that led to this report was, apparently, that not enough storage was provided in CERP. Is it or not? "Adaptive" management is a reasonable approach.
36. The report does not support recommendation 6, whether is true or not.

There are a few very general comments to be made. First, the AMO relationship to rainfall is weak. Basing engineering design and planning on it may indeed be stretching it. But, the arguments and presentation of the report are not strong. The seasonality shifts in inflows to the lake are the least studied and the key to the

conclusions. The reasons for increases in inflows must be understood. Second, the focus on AMO versus ENSO for operations is not justified. Third, the discussion of planning and management models makes sense only to those that know the models. It is necessarily a superficial discussion. Fourth, doing some sensitivity and uncertainty analysis is important. Fifth, the report seems ambivalent, on the one hand arguing that uncertainties in climate indicators predictions are too high to make them useful and on the other hand arguing that they are being used in a variety of ways. Finally, the data seems to suggest that the seasonal effect of AMO may be very well tied to tropical storms. This needs to be fleshed out before embarking on approaches like synthetic generation of precipitation.

Areas not covered by the review

References were not checked. Formats and district standards were not checked. Review is strictly based on material provided, and scope of work.

Typos and format

1. Page A-iii, first paragraph should read Pacific Decadal Oscillation
2. Axes or caption of Figures 7 and 8 are wrong.

Submitted July 3, 2006